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The Geological Progress of Twenty-five Years

President's Address, before
the Ohio Academy of Science,
November, 1911

By LEWIS G. WESTGATE

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The Geological Progress of Twenty-five Years

PRESIDENT'S ADDRESS

BEFORE THE

OHIO ACADEMY OF SCIENCE

NOVEMBER, 1911

BY

LEWIS G. WESTGATE

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The Geological Progress of Twenty-Five Years

LEWIS G. WESTGATE.

OUTLINE

INTRODUCTION.
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THE GEOLOGICAL PROGRESS OF TWENTY-FIVE YEARS.

INTRODUCTION.

The feeling of elation which follows one's reception of the news of his appointment to the presidency of an organization like our own is but momentary. It gives place almost at once to a feeling of anxiety as one begins to wonder what subject he shall choose for his presidential address a year later. In the present case I have decided to depart from the custom of recent years, of presenting a piece of research work, which is almost necessarily technical in character, and to take a more general subject, in the hope that such a subject may prove more interesting to the society as a whole; and I do this the more readily since the majority of our members are biologists and not geologists. I shall therefore speak of some of the advances which have been made in the science of geology in the last twenty-five years; for it is almost that time since as an undergraduate at Wesleyan University it was my privilege and my delight to take my first course in geology under Professor W. N. Rice.

In carrying out this purpose I limit myself to the geological principles and theories which have come into prominence in the time under consideration. Detailed studies in the field, laboratory or office are interesting and important, and in many cases mean long months or years of the closest kind of work, but, after all, the general principles which are based on them are the important contribution to geology, and it is these which are of the largest interest to the general student. He wishes, for example, to have a correct theory of mountain formation, but does not care for any greater knowledge of the geological detail than is necessary for a clear understanding of the theory.

I also find myself under two limitations not of my own choosing. The first is the difficulty, the impossibility, indeed, of escaping my American point of view and prejudices. This is more difficult to do in geology than in any other science, because geological progress is more independent in different nations and continents than is progress in the other sciences. And the reason for this greater independence lies in the very nature of the science. The chemist and physicist, even the zoologist and botanist, unless engaged in systematic studies, are working on the same materials the world over, and are interested in each other's detail. But the material with which the field geologist deals varies widely in different countries, and the detailed work of geologists is different for this reason. The peculiar geological history of the North American continent, for example, offers problems to the American geologist in large measure unlike those in any other part of the world. It determines the character of his detailed studies, his geological principles are in considerable part developed from his own field studies, and his geology progresses in semi-independence of the work abroad. Not wholly so, of course, for the workers in every land are alive to the general results obtained elsewhere. But the inevitable result is that the work of one's countrymen bulks largest in one's everyday thought, and unconsciously emphasizes the relative value he places upon their work. Only one who is widely acquainted with foreign literature and work can get away from this limitation.

The other limitation I find myself under is the difficulty of deciding how the state of knowledge of a science at any one time is to be judged. From the best school texts and from the larger manuals? But these, and especially the former, are often some years behind the time, even at the date of their issue. From the views current among the workers in the science? It is not always easy to know what views are current. In many cases matters are in debate, still to be fought out to a conclusion. And if it should be the case that some principle, found later to be of great value, has been published but has not found its way to acceptance by workers in the science, can that principle be counted among the possessions of the science at the time? Twenty-five years ago, in 1886, Le Conte's *Elements of Geology* and Dana's *Text-book of Geology* were the common college texts, and Dana's *Manual of Geology* the only American reference manual, while Geikie's *Text-book of Geology* was the leading English text. It is to these books and to the reports current at that time in the national and state surveys that I go to learn the state of geological opinion.

Before speaking of the new principles which have found their place in geology in the last quarter-century, it is well to call attention to some facts which show the great growth of the science as a whole; a growth not peculiar to geology, but paralleled in the other sciences. In many of the colleges which now have separate departments of geology, the teaching of geology was then combined with that of other subjects; and in other colleges and universities which then had separate departments of geology, these departments have now grown in instructors and resources. In many states geological surveys were then in existence and doing good work. This work in most cases has grown, and in some cases new surveys have been started.

The earlier western surveys of Hayden, Powell and King were replaced by the U. S. Geological Survey in 1879; and the magnificent series of reports published by the survey make America's greatest single contribution to geology. In 1882 the Geological Survey had been directed to prepare a geological map of the United States. This necessitated a topographic base map, which was at once started. In 1886 the Survey had published maps of a small area in the Appalachian mountains, and of one or two small areas in the far west. Today hardly any considerable part of the country is wholly unmapped, and the work is being rapidly pushed to completion. The influence of this work on the progress of geology it would be hard to estimate. The Survey began publishing the geological map in the form of folios in 1894; and to date nearly 200 folios have been issued.

In 1886 no national geological society existed in the United States, and no magazine devoted exclusively to geology. Both of these deficiencies were soon made good, for in 1888 the Geological Society of America was founded, and the publication of its *Bulletin* was commenced. In 1888, too, the *American Geologist* was started, to be continued after 1905 as the *Economic Geologist.* In 1893 the first number of the *Journal of Geology* was issued.

Today the texts of Dana and Le Conte have been somewhat generally superseded. Scott's *Introduction to Geology* and Chamberlin's and Salisbury's *College Geology* are the common college texts, while the American manual is Chamberlin's and Salisbury's *Geology* in three volumes, which, in the opinion of many, is the leading text of today in its presentation of the principles of the science.

THE STUDY OF LAND FORMS.

In no department of geology has greater progress been made than in the study of land forms. Geology is the history of the earth. That history, until recently, has been read almost exclusively, from the rocks, from their distribution, structure and fossils. But we now know that the history of later geological time can be read also from the forms of the land surface, that streams in particular shape the land to characteristic forms, and that these forms in turn can be used to give us the history of the land's surface. The older geology, dealing with rocks primarily, was largely the study of former areas of deposition, of water bodies. Today geology has, in the study of land forms, a means of supplementing this knowledge of water areas with the history of land areas.

Base-level, peneplain and erosion cycle.[1]

The importance of stream erosion had long been recognized, yet when Powell[2] in 1875 first formulated the idea of the base-level, he was stating what may have been implicit in the statements of earlier writers, but what had not been definitely recognized and defined. A base-level may be defined as an imaginary surface, the projection beneath a drainage basin of the surface of the water body into which that drainage basin empties. Towards this imaginary surface the streams cut their valleys; near their mouths they may actually reach it; but further back they cannot, because of the necessary though, in the case of the large rivers, small rise of the stream up-valley. After the nearest approach to base-level has been made, the widening of the valleys by valley-side weathering and lateral swinging of the streams still goes on, the inter-stream areas are attacked, and the whole land surface is slowly reduced to a low rolling plain not far above base-level. Plains of this origin were recognized by Powell[2] in 1875, in the level surface of tilted rocks on which the Carboniferous limestone of the Grand Canon rested, and similar buried or "fossil" plains were recognized later by Van Hise and Walcott. McGee[3] in 1888 was the first to call attention to such a plain, dissected but existing as a land form today, in the Middle Atlantic slope; and Davis followed with an account of similar plains as developed in Pennsylvania, New York and New England. It was Davis[4] who in 1889 first applied the name *peneplain* to this end-form of long continued subaerial erosion. The time during which the land surface is passing from the initial form of youth to the peneplain which is developed in old age is the cycle of erosion and it is

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[1] See Davis: *Geographical Essays,* (Ginn & Co.), pp. 249-513.

[2] Powell: *Exploration of the Colorado River of the West.*

[3] McGee: *Three Formations of the Middle Atlantic Slope.* Am. Jour. Sci., xxxv., pp. 141-2.

[4] Davis: *Topographic Development of the Triassic Formation of the Connecticut Valley,* Am. Jour. Sci., xxxvii., p. 430.

Davis who, more than any other one person, has given definiteness to this conception of the geographical cycle.

It has been stated that the existing peneplains of eastern United States are not intact, but have been dissected since their formation. In central New England, for example, the even sky line of the plateau, which stands two thousand feet above sea-level, represents an old peneplain, which was later raised, and into which streams have cut their valleys to a depth of more than a thousand feet. In many cases broad lowlands, themselves peneplains of a later cycle, have been cut below earlier peneplains. It is evident that the present position of such peneplains gives us important information regarding the amount and character of the successive uplifts of the region. For if such plains were made near base-level, and are now found far above base-level, the difference in height measures the amount of uplift.

In the case of the Appalachians, the earliest peneplain has been arched up to a maximum of over four thousand feet above sea-level. But in other mountain areas old peneplains are recognized by the evenness of sky-line, at much greater heights. Portions of the Cascade range of central Washington are cut from an eight thousand foot plateau which was a peneplain formed in Pliocene time.[5] In other mountain ranges similar conditions have been found. The bearing of these facts on mountain origin is evident. These mountains have the folded and disordered structure typical of mountain areas, but the original high relief which this disordered structure suggests had been reduced to a peneplain before the present relief was produced. In the case of both the Appalachians and the Sierra Nevada, the range of today is the second which has stood on that site, and we have evidence, in the peneplain from which the later range is cut, of complete destruction by erosion of the earlier range.

Our knowledge of peneplain development in the past brings out with great clearness another great fact of geology, the importance of which is only recently fully recognized, namely the alternation of periods of crustal disturbance with long periods of crustal quiet. Peneplain production requires that land should remain for a long period in about the same position in regard to base-level. If the land surface is being reduced one foot in five thousand years, and if the average height of a continent after uplift is two thousand feet, the production of a peneplain would take ten million years. This value is suggestive only. Towards old age the rate of downward erosion becomes much less, and, on the other hand, in but few cases are peneplains developed over a whole continent. Yet the value is true in the sense that the times of quiet must be measured in terms of millions of years. While separating these periods of relatively stable crust have been the periods of crustal movement of rock folding, and faulting, and of up-arching of broad areas.

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[5] Smith and Willis: *Contributions to the Geology of Washington,* Professional Paper No. 19, U. S. Geol. Survey, plates 8 and 19.

The dissected upland plains which we have been describing were recognized in the mountainous parts of Great Britain, and were explained by Ramsey and later English geologists as plains of marine denudation, which had been later raised and dissected. Evidently we have here a problem in psychology. To the Englishman on his wave-girt island, with its relatively small river systems, sea action was more noticeable than stream work. The American geologists of the western surveys had the erosive work of rivers before them on a stupendous scale. There is of course today abundant reason for believing that peneplains are of subaerial and not of submarine origin; and this explanation is now accepted by English geologists for their "plains of marine denudation," but it is interesting that each country had a theory suggested by its own geological environment.

GLACIAL GEOLOGY.

The progress in glacial geology in the last twenty-five years has been very great, both in itself and in its bearing on general questions. I shall speak of three main lines of work:

1. The subdivision of the Pleistocene.
2. Former glacial periods.
3. Theories of the glacial period.

The Subdivision of the Pleistocene.

Twenty-five year ago the subdivision of the Pleistocene glacial period was not generally recognized. Le Conte (*Elements* *of Geology,* 1882) had a map of the drift area in which the extreme southern border of the drift was shown, and within that the morainic system of what we now know as the last or Wisconsin drift; but there is no suggestion of more than a single ice-advance. But at the time Le Conte was writing, the geologists of the upper Mississippi Valley were already engaged in the field studies which were to show that the glacial series was more complex than had at first been supposed. Chamberlin and Salisbury, in their paper on the Driftless Area (1885), give an outline of the glacial history of the interior, in which two epochs are recognized, without emphasizing at all the length or significance of the interval between them. In the years that followed an active discussion went on as to the meaning of this two-fold division. Does the "interglacial epoch" signify a slight retreat of the ice, with a later re-advance, in which the Wisconsin kettle-moraine was deposited? or does it mean the disappearance of the ice from the region, perhaps from the continent, and later the invasion of the northern states by an entirely new ice sheet? The outer till is much more deeply weathered and more dissected. and in some cases it is possible to show that it was in this condition at the time of deposit of the inner drift. More than this, the differences in the two drift sheets indicate that the ice-sheets that formed them advanced in quite different manner, and over land surfaces at unlike altitudes above sea-level. These things suggest a long interval and changed conditions between the two ice advances, and create a strong probability for separate ice-sheets.

The same field study has brought out the further fact that there are more than two drift sheets and ice advances. Chamberlin and Salisbury (*Geology,* III. 413) give six, which, commencing with the most recent, are Late Wisconsin, Early Wisconsin, Iowan, Illinoian, Kansan, Sub-Aftonian. The genuineness of the Iowan has been recently questioned by Leverett, and even among Iowan geologists, there is doubt as to its real meaning. Estimates of the relative weathering and erosion of the different sheets indicate roughly that, if the time since the commencement of the retreat of the late Wisconsin ice-sheet is taken as 1, the time since the retreat of the early Wisconsin is 2, of the

Iowan 4, the Illinoian 8, and the Kansan 16. The estimates of the time since the beginning of retreat of the late Wisconsin ice vary between 20,000 and 60,000 years. Using the relative values for the time of the earlier ice-sheets given above, this would make the age of the Kansan between 300,000 and 1,020,000 years, and even here we are not at the actual beginning of the Pleistocene for this does not carry us back to the Sub-Aftonian. These facts and values are suggestive only, but they are sufficient to show that the ice age was a very long and a very complex period, the successive ice advances being separated by long inter-glacial periods in which the climate may have been as mild or milder than today; indeed, it is not certain that we ourselves are not living in an interglacial period. Earlier glacial studies, proceeding on the theory of a single ice age, placed together in many cases events which we now know belong to different ice-advances. but geologists have come to see that the unravelling of the events of the Pleistocene means an immense amount of detailed work, which will occupy many years to come.

In eastern United State the late or Wisconsin drift extends to the southern border of the glaciated area. It is not until central Ohio is reached that any considerable width of the older drift shows outside the Wisconsin area. This outer sheet of older drifts widens to between 200 and 300 miles in the upper Mississippi valley in Iowa, Illinois, and neighboring states. It is the magnificent development of the older drifts in the interior which made it inevitable that the geologists of these states should take the lead in reaching a true understanding of the complexity of the glacial period, in spite of the fact that in the east and in New England long and detailed study had been given the glacial deposits by able geologists. We have here another illustration of the control of local phenomena over the development of the science.

Former Glacial Periods.

Ramsey, in 1855, in a paper "On the occurrence of angular, sub-angular, polished and striated fragments and boulders in the Permian breccia (of England) and on the existence of Glaciation and Icebergs in the Permian epoch" was the first geologist to announce glaciation previous to the last or Pleistocene epoch. Geikie (*Text-book,* 4th ed., 1893) states that "the evidence now accumulated in South Africa, India, Cashmere and Australia seems to point to some general operation on a gigantic scale in the southern hemisphere at the close of the Carboniferous or in the Permian period, whereby boulder beds were produced and limestones and rocks *in situ* were polished, striated and grooved," proving "the occurrence of a former ice-age in later Paleozoic time, which rivalled in its extent and seemed to have surpassed in the magnitude of its deposits, the glaciation of the northern hemisphere" in pleistocene times. "From the fact that the boulder beds are intercalated among marine strata it is clear that, to some extent at least, the ice reaches sea-level. We are still in ignorance, however, of the position of the high grounds from which the ice sheets descended." Twenty-five years ago, the attitude of geologists towards Permian glaciation was an attitude of scepticism. Even in 1893, Geikie (*Text-book,* 3d ed.) would say only, of the glacier-like deposits of the English Permian, that "Ramsey had no doubt that they were ice-borne, and consequently that there was a glacial period during the accumulation of the Lower Permian deposits of the center of England." And the American texts of the time were silent on the subject. Today, however, Geikie's later statement is generally accepted as true. And from Norway and China there comes similar evidence of glaciation which is referred "either to very early Cambrian or to pre-Cambrian time." (Chamberlin and Salisbury, *Geology,* II, 273.) It yet remains to be seen whether evidence brought forward for a still earlier Huronian ice age will be generally accepted.

The importance of Carboniferous and Cambrian glaciation lies not so much in the fact itself, interesting as that may be, as in its bearing on general geological theory. Clearly, with these remote periods of glaciation, there can have been no general progressive climatic change throughout geological time, from an earlier warm to a present cooler condition. Rather, there has been from time to time a recurrence of colder periods, and climatic changes have been in the nature of swings to one side or the other of a mean, rather than an evolutionary progress in one direction. This fact must be considered in any theory of the cause of glaciation.

The Cause of Glacial Periods.

In the middle eighties, various theories of the cause of the glacial age were held, though the only one which had anything like common acceptance was Croll's Hypothesis. Croll's hypothesis is primarily astronomic, and secondarily geographic. At present, winter in the northern hemisphere comes when that hemisphere is nearest the sun (perihelion). 10,500 years hence, due to the precession of the equinoxes, northern winter will occur when the earth is at aphelion. But the eccentricity of the elliptical orbit of the earth slowly changes, under the attraction of other planets on the earth. Today the eccentricity is slight. At other times it has been and will be greater. Croll's hypothesis assumes that the glacial period will occur in the northern hemiphere when that hemisphere is turned from the sun in aphelion at a time of great eccentricity. Aphelion winter recurs every 21,000 years; periods of maximum eccentricity are much longer and more irregular. Certain other changes, such as the shifting of the heat equator, and of the equatorial current, and a consequent variation of the proportion of that current turned into northern and southern latitudes. were believed by Croll to work with the astronomical factors.

If Croll's hypothesis is the true explanation of the glacial period, we should expect (1) the recurrence of glaciation many times in the history of the earth; (2) that in any one period there would be several alternations of glacial and non-glacial conditions, with intervals of 10,500 years between the culminations of successive glacial periods, the development of each ice-sheet occupying perhaps not more than five to six thousand years; (3) that glacial conditions in the northern and southern hemispheres would alternate. Glacial field studies have shown, on the contrary, that the ice advances were vastly longer than Croll's hypothesis admits of, and that the intervals between them do not correspond with the theory. Chamberlin summed up the situation when he wrote: "I think I speak the growing conviction of active workers in the American field, that even the ingenious theory of Croll becomes increasingly unsatisfactory as the phenomena are developed into fuller appreciation. I think I may say this without prejudice, as one who, at a certain stage of study, was greatly drawn towards that fascinating hypothesis."

Within the last fifteen years a new hypothesis has been proposed by Chamberlin. This was first fully presented in 1899 in a paper entitled "An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on the Atmospheric Basis." This "atmospheric hypothesis" of Chamberlin's is not simpler than Croll's, but explanations of geological phenomena are not commonly simple. Indeed there is a certain parallelism between the successive steps of this theory and the succession of cause and effect in Darwin's famous illustration of the way in which "animals and plants, remote in the scale of nature, are bound together by a web of complex relations." You will remember that with the increase of cats there went a decrease of field mice; this meant an increase of humblebees (the field-mouse's victim), and a corresponding increase of clover, which is cross pollenized bv the bees. Therefore. the more cats the more clover. Huxley, I believe. extended the series by one term and showed that maiden ladies, through their fondness for cats, belonged in the chain. Even more complex is the atmospheric hypothesis. Primarily it explains glacial climate by the loss of CO_2 from the atmosphere. Air containing CO_2 has greater power of absorbing heat, the waves received from the sun and especially the waves of greater length radiated from the earth. Less heat is thus lost to space and the temperature of the air is higher. With loss of CO_2 there is a smaller amount of heat retained and a lowering of atmospheric temperature. This in turn decreases the amount of water vapor in the air. which, after CO_2 is the atmospheric constituent with the greatest heat-absorbing power.

The hypothesis assigns the loss of CO_2 to land elevation. This is followed by increased weathering, both because of the increased land area exposed to weathering, and because of the greater thickness of the zone of weathering, the ground water level being lowered as streams deepen their valleys, following the uplift. The abstraction of CO_2 from the air in the process of changing carbonates to bicarbonates which accompanies the chemical weathering of limestones. and in the formation of carbonates by combination with the oxides of calcium, potassium and sodium in igneous and metamorphic rocks, is the primary cause of colder climates which bring on glaciation. The diminished area of the continental shelf which follows uplift, with the consequent lessening of lime-secreting organisms, would mean less CO_2 returned to the water and air, as a result of changing the bi-carbonate of lime to the carbonate in shell making. The general theory has consequences in a number of directions which cannot be followed here.

The recurrent glaciations are explained as due to the combination of primary and secondary causes pushing their effects beyond a condition of equilibrium. so that a reaction ensues, bringing on the milder conditions of an interglacial epoch, the conditions swinging to one side or the other of a mean until the general cause has worked out its effect.

The peculiar localization of Pleistocene continental glaciation in North America and Northwestern Europe is attributed to the control of the great area of low atmospheric pressure located over the North Atlantic, upon the winds of the two northern continents.

This statement is wholly inadequate as a description of the atmospheric hypothesis. But I am not attempting to describe that hypothesis fully. I have not time to do that. and it would not accord with the purpose of this paper. But I think I have said enough to indicate in general the *kinds* of agencies which Chamberlin believes were responsible for glaciation. They are fundamentally atmospheric, changes in the amount of CO_2 in the atmosphere. The cause of these changes was geologic, land uplift followed by erosion and weathering. The theory is proposed as a working hypothesis. It has been elaborated in great detail by its author. It seems to be a better explanation of glaciation and of glacial history than any of its predecessors, but it may take many years of study before it becomes established, perhaps much modified, as the accepted theory of glaciation. In the meantime it largely dominates thinking in this. which has been for a half-century one of the chief fields of geological speculation.

THE INTERPRETATION OF THE SEDIMENTARY ROCKS.

Twenty-five years ago the petrographic microscope was a new tool in the hands of the geologist, and with its use there came a renewed interest in the older igneous and metamorphic rocks. One could read between the lines that the problems of the sidementary rocks, of their structure, origin and significance, had been largely solved. Yet one can now believe that since that time geological theory has been advanced at least as much by what we have learned from the story of the sedimentary as from the story of the crystalline rocks. And this in two distinct lines.

Fluviatile, Lacustrine. and Marine Deposits.

In most discussions of the origin of the shales, sandstones and conglomerates of the older periods, it has been assumed that they are of marine origin, the coarser portion near the shore, the finer farther from the shore line. The similar non-marine Tertiary rocks in our western states the geologists of our western surveys, and after them the texts, have until recently unanimously considered lacustrine, deposits in lakes in some cases thousands of square miles in area, and larger than any existing American lakes. Dana (*Manual*, 1895) speaks of "the great lakes of the early Tertiary—the Eocene—in the Rocky Summit region" and "the later Tertiary lake basins either to the east or west of the Summit region." And this was done, curiously enough, in spite of the fact that these same workers found in the field and correctly interpreted the extensive deposits of gravel, sand and clay which are today being formed along the base of mountain ranges where streams find their velocity checked as they issue from their mountain-valley courses and are compelled to lay down their load in the form of broad alluvial cones or fans, which merge at a slight distance from the mountain into one piedmont alluvial slope fronting the whole range. In spite also of the fact that in many regions of the world, along the northern base of the Alps and Pyrenees, over the river plains of northern India and eastern China, such deposits are forming today or have been formed in the past on a scale comparable with that of the Tertiary areas of supposed lake deposits in the west. It would seem to have been the unthinking acceptance of a traditional interpretation, which assumed that such sedimentary deposits must have been made in standing water. But little thought is needed to see that sands, muds, and gravels are deposited subaerially by streams, as well as in lakes or seas.

Davis called attention (1900) to the situation, in his paper on *The Fresh Water Tertiary Formations of the Rocky Mountain Region.* He here showed that Penck (1894) had already recognized the possibility of the occurrence of fluviatile (continental) deposits in the older rocks, and that Gilbert had already applied this interpretation to some of the "lake" deposits of the western plains area. Davis raised the whole question, suggested criteria for discrimination between fluviatile and lacustrine deposits, challenged in particular two of the western Tertiary formations, and put the whole matter on the modern basis. Since then it has been necessary to treat each formation as a separate problem, and the conception of the Tertiary as a "lake" age has rapidly waned. Rather it was a time of extensive plains building along mountain bases and over intermont basins by overloaded depositing and wandering streams on leaving the mountains.

Another result of this discussion has been to raise a doubt as to the origin of many of the non-fossiliferous or sparingly fossiliferous deposits in other parts of the country; deposits of the older periods which formerly were unquestionably accepted as marine; and some of these formations are now coming to be recognized as in part or in whole of subaerial origin, the deposits of rivers. In the second case as in the first, we are driven to a revision of our conception of past geography, for we can no longer consider sandstones and shales, *ipso facto,* as indicating the former presence of water areas.

PALEOGEOGRAPHY.

Another recent development we owe largely to the paleontologists. In the last twenty-five years an immense amount of detailed paleontologic work has been done in this country. Description of new species has been an insignificant part of the work. It has concerned rather faunas, the comparison of faunas of different regions, and the movements or migrations of faunas. This has also required study of the rocks in which the faunas were found, and the use of all possible means to determine areas of past seas, and barriers to and lines of migration. The study of the geography of the past has always been a part of geology, but in recent times the subject has assumed so much importance that it has received the dignity of a name of its own, and we now have paleogeography, paleogeographers, and paleogeographic maps of the different geological periods. Two recent works of the highest value summarize what has already been done, and form the basis from which all work in the near future must be built. These are Schuchert's *Paleogeography of North America* (1908) and Ulrich's *Revision of the Paleozoic Systems* (1910).

Schuchert's paper gives an unrivalled series of paleogeographic maps of North America. Both papers discuss in detail the principles of paleogeographic interpretation and their application to American formations. They give in far greater detail than has been given before the behavior of the continental surface in geological time; the advances and retreats of the sea, and the shiftings of areas of erosion and sedimentation. One important result of this work is to change somewhat the idea which was current a couple of decades ago in regard to continental growth, especially the growth of the American continent. It was Dana's belief that there had been a sort of evolution of the North American continent from the middle or later Cambrian, when the present continental surface was largely submerged, except for land areas in Canada, along the crystalline area of the Appalachians and in the west, to the present condition of continental emergence. Not of course a steady progress, rather a progress with relapses; and yet after each great period of geological revolution a nearer approach to present conditions. Recent stratigraphic studies indicate no such progress: instead, at different times, some later and some earlier even than the beginning of the Paleozoic, land area was as great if not greater than today; while alternating with these periods of land extension and often of high relief, were periods when the lands were worn towards base-level, and widely covered by shallow sea. Periods of emergence have alternated with periods of submergence since very early times; and certainly since the commencement of the Paleozoic we find no support in geology for a continental evolution at all analogous with what is found in the realm of animal and plant life.

LARGER PROBLEMS OF GEOLOGY.

Geology is the history of the earth; of the whole earth, both shell and core. We are coming to see more and more clearly that the changes which we can see going on today over the earth's surface, and which we can infer from the study of the rocks of the earth's crust, are but expressions of changes which are going on within the great mass of the earth, forever hidden from our sight. They are symptomatic of deeper and more fundamental changes. And our geological theory will never be complete until we have clear and correct notions of the history of the earth as a globe, and of the changes which are now going on within it. We have not that knowledge today, it may be years in coming, and in the mean time our writing on these fundamental problems will be highly speculative. Yet such speculation is a necessary part of our science in each advancing stage. If it is carefully controlled by known geological fact, no harm need come. Along two lines of geological theory important advance has been made in recent time.

Continents and Mountains.

The condition of geological thought on the subject of the permanence of continents and ocean basins in the middle of the nineteenth century is well summarized in Tennyson's lines:

> There rolls the deep where grew the tree.
> O Earth! what changes hast thou seen!
> There where the long street roars, hath been
> The stillness of the central sea.

This would mean that there has been no permanence of ocean and continent through the past, that they have been interchangeable. Present opinion however is that continents and deep sea have held in general their present position since the beginning of Paleozoic time, and probably much longer. This is not to deny that at times the present continents have been widely submerged beneath ocean waters, for this has happened again and again. But these epicontinental seas, as they have been called, have been shallow, and the real continental border has been at the edge of the shallow circumcontinental terrace, where the relatively steep slope to the oceanic abyss commences. The continental seas have been the overflowings of the over-full ocean onto the continental platform, so that a modern geological editor, sacrificing beauty to truth, might change Tennyson's lines to read:

> There where the long street roars, hath been
> An epicontinental sea.

One reason for this belief in the permanence of continents is the fact that the marine deposits now showing in the rocks of the continent are essentially shallow water deposits. Even limestones indicate clear rather than deep water. Another line of evidence, developed more recently, lies in the fact, determined by gravity measurements and plumb-line deflections, that the earth mass beneath the continents is of less specific gravity than beneath the oceanic basins. Indeed these measurements show that a practical equilibrium exists between the oceanic and continental segments of the earth, the greater length of radius of the continental segments being compensated for by their less specific gravity, the masses of the two being the same, area for area. If continent and ocean basin are in this isostatic adjustment, the great difference of relief between continental platform and ocean basin is evidently an expression of differing density beneath the surface. It is difficult to see how the difference of density could essentially change, and we have therefore a permanent cause of difference between continent and ocean.

This fact of the essential permanence of continental platform and ocean basin, taken in connection with the shrinking of the earth following cooling and condensation, gives the starting point of geological processes. The earth is divided into continental and oceanic segments, the former lighter, the latter heavier, the former making one-third of the earth's volume, the latter two-thirds. The sinking of the larger heavier oceanic segments crowds the continental segments. This results in a squeezing up of the continental segments, of which we find evidence in the repeated uplift of the land surface. The folded and faulted mountain ranges, which occur principally along the borders of continents, are due to the lateral thrusts and readjustments along the borders of the segments.

The continental platforms have therefore been described as positive elements, the oceanic basins as negative elements. Within each, on a smaller yet absolutely large scale, positive and negative elements occur. The continental platform does not act as a unit, if we consider detail. There have been areas of repeated uplift, positive elements, of which one of the best known is the area of crystalline rocks along the Atlantic sea-board known as *Appalachia*. Through geological history it has been a land area, frequently renewed by uplift, almost always subjected to erosion. While to the west of it the Appalachian trough existed during Paleozoic time as a negative element, an area of repeated subsidence and of deposition. And yet both, in a large way, were but parts of the larger unit, the continental platform. This subdivision of the positive continental areas into positive and negative elements of a lower order is a direct inference from the field studies.

But these views which I have summarized are new. Le Conte in his *Elements of Geology* had a beautifully clear and adequate state of the immediate conditions which led to the formation of a folded mountain range like the Appalachians. But his theory did not apply equally well to ranges like the Sierra Nevada, which is a great block of the earth's crust tipped up along its eastern border, nor to ranges like the Big Horns and Black Hills, cut out of broad, more or less flat-topped up-bowings of the crust, nor to block uplifted plateaus. Le Conte assigns the differences between continents and ocean basins to unequal radial contraction of the earth in its secular cooling but does not suggest that conditions producing and maintaining continents may also be responsible for mountains. Geological theory today makes this contrast of continent and ocean basin the fundamental fact of earth structure. Successive continental uplifts are due to the crowding of the continental segments between the heavier oceanic segments. Folded mountains are due to the yielding to the accompanying lateral thrust. While plateaus, plateau-like folds, and areas of sinking within the continent are due to re-adjustment between blocks of a second order which make up the continent.

The Planetesimal Hypothesis.

In our school days, as in those of our fathers, the Nebular Theory of La Place held unchallenged sway. This hypothesis is so familiar that I need not state it. Objections to this hypothesis, mechanical, geological and astronomical, have seriously shaken confidence in it. Within the last twenty years a substitute for it has been proposed by Chamberlin, the planetesimal hypothesis. Like the La Placean hypothesis it is a nebular hypothesis. But in so far as it concerns the growth of the earth the two are almost antipodal. On the La Placean hypothesis the earth reached its present size by the cooling and shrinking of a vastly larger hot gaseous mass, and on George Darwin's modification of the theory (the meteoric hypothesis) it is probable that the meteorites would be vaporized early in the earth's growth by collisions; on the new, it grew to a maximum by the in-fall of planetesimals, cold bodies of various size moving in elliptical orbits about the sun, and added to the growing mass of the young earth. Long before the beginning of the Paleozoic the heavens had been cleared in the region of the earth's orbit, of these scattered masses, and the earth had reached its maximum size. Today it is condensing, perhaps as a result of loss of heat, perhaps under the attraction of gravity on its mass. The interior heat of the earth. on the newer theory, is generated by condensation, and is not an inheritance from the nebula.

The planetesimal hypothesis gives a wholly new explanation of the atmosphere and of the oceans. The material of both was brought in dissolved or occluded in the infalling planetesimals, just as now the meteorites contain three to four times their volume of dissolved gases. With the later development of high temperatures these gases made their escape to the surface, and after the earth became large enough for gravity to overcome the tendency of the gaseous molecules to fly off into space an atmosphere and later a hydrosphere came into existence. In this case oceans may have existed on the earth well before it attained its present size. Life, too, may have come into existence at a very early stage, as soon as conditions of air and water were such as to support living beings.

The future of the Planetesimal hypothesis no one can forecast. The hypothesis of La Place held almost undisputed sway for a century. Perhaps this newer one will in its turn pass. Or, more or less modified, it may finally establish itself as a better statement of our earth's earlier history. I wish only to point out that it gives a fundamentally different interpretation of this earlier history and of the forces which are at work within the globe as a whole, and that this new interpretation must modify, indeed, already has modified our thinking on the great fundamental problems of geology. Our conception of the causes of volcanic action, of all movements of the earth's crust, from the great periodic readjustments between continents and oceanic segments, with their accompaniments of mountain folding and plateau formation, to the slighter movements which are constantly present, of the origin of the great complex of rocks which makes up the Archean;—our conception of the causes of all of these and of many minor geological matters, is profoundly different on the two hypotheses. Many of them seem much clearer on the newer than on the older theory.

IN CONCLUSION.

Several matters of human interest come out in the progress of this paper. I have already mentioned the control which local geological phenomena exert over geological theory, in speaking of the different agents which English and American geologists assigned as the cause of peneplains. The same influence of local conditions was seen in the greater readiness of the geologists of the Mississippi valley to accept the divisibility of the glacial period. The work of Ulrich and Schuchert grows out of the appeal which the paleontological richness of the Cincinnati rocks early made to them. The illustrations of this principle are many in all geology, both in that of recent time in America and in the earlier days of the science, as everyone knows who has read Geikie's *Founders of Geology.*

The importance of American contributions to recent progress I have not spoken of separately. This has already been well done by Professor Rice in his paper, *The Contributions of America to Geology.* I need only state my belief that of the different subjects I have treated in this paper, the influence of American geologists has dominated in the following: the study of land forms, the subdivision of the Pleistocene, the recent interpretation of sedimentary rocks, and speculation on the larger problems of geology, in what may be called the philosophy of geology.

Finally I should like to call attention to the great part speculative geology now plays in science. Great as has been the detailed study in field and laboratory in the last quarter-century, the part of geological speculation has been even greater. To some minds this may not be a matter for congratulation. They may prefer study of the facts without generalization, certainly without speculation. The late Professor Lesley of the Pennsylvania survey was one of these. In his *Final Summary,* speaking of what he considers unjustifiable speculation. he writes:

"Our knowledge of details is so poor that all our general statements are mere mutterings in sleep, or the incoherent rhapsodies of fever. The world is a kaleidoscope; at every touch it turns a little, and the scheme of shapes and colors changes to our eye. A malaria of the indefinitely complicated exhales from every region of geology and attacks the wise and simple alike. Happy the investigator whose intellectual constitution is not ruined by it in the end. Foolish generalizer, who mistakes the paroxysms of chill and fever in his own incompetent imagination for an actual rhythm in the energy of the world of which he dimly sees but the part of a part."

But recent time has shown that most geologists do not agree with him. And it is very interesting to see how different geologists, from widely separated points of departure in actual field work are led on to speculation regarding the deeper problems of geology. Willis, from stratigraphic study of the folded Appalachians, and Ulrich and Schuchert, from the study of Cincinnati fossils, go on step by step to the consideration of problems of continental development. While Chamberlin, starting with his field studies on the Pleistocene, is led by the logical necessities of his work to frame a more satisfactory theory of the cause of glaciation and still further to an all-embracing theory of the origin of both atmosphere and the earth which carries it. As of old all roads led to Rome, so it seems as if todav all lines of field work, followed to their logical end, lead to speculation as to the larger processes of geology.

I believe that this is best, this combination of detailed field and laboratory study with speculation; best both for the science and the individual. The field studies furnish both the problems for speculation and the control over it, while geological theory stimulates field study by pointing directions of profitable endeavor. And I believe too that a division of thought between detailed studies and speculation on the larger processes of the science is most stimulating to the individual. Certainly all of us have derived both pleasure and profit from the broad range of vision which the newer generalizations have opened up.